Department of Genetics,
University of Wisconsin,
Madison 6,
Wis.

July 27th., 1954

Dear Josh,

You asked for some opinions on Uetake's paper. I've now had a chance to re-read the paper so here are the opinions, they are rather general and not directed to any specific faults or obscurities.

The paper is undoubtedly too long, this is largely due to the over-extensive historical introduction. This I feel could be overcome by reference to review papers, particularly as there is some reason in the discussion. The numbering of paragraphs, or findings, irritates me, but I presume the editors would cut this anyway. Much of the experimental findings could be telescoped, there is no need to devote a new section to each individual sero-type. This same criticism applies to the break-down of the discussion. I feel, moreover, that such a rearrangement of material would make it far more easy to follow.

Section 15) of the discussion and Table 3 I find rather difficult to swallow. As far as I can see there is an overall similarity in origins of all the strains listed. This seems to be some form of special pleading which tends to detract from the rest of the paper.

The data on the incorporation of the 15 'determining' factor is interesting, also the fact that the potentiality to produce 10 is not lost but apparently only concealed, in some cases not even that. What is the state of those strains (section IX) which have been reverted from 3,15 to 3,10 ? Are they still lysogenic? Surely if there were a very small fraction of 3,10 cells (nonlysogenic) in the transformed culture these would be at a selective advantage in the presence of a 15 containing medium, this does not require the postulation of a direct anti-phage action on the pro-phage as is suggested in 13) of the discussion. In any case the sera could be pre-absorbed with a phage preparation so eliminating this possibility.

There are, of course, faults in English but these are not very extensive and I wont bother to detail them. I presume you will take this up with Uetake yourself.

A culture of Aerobacter has come from Hinshelwood. Do you wish anything special done with it? I've only had it subbed into a stab as yet in case the original slope dries up.

Little of note has happened in Madison since your departure. I forewarded some mail to Ann Arbor and have stockpiled some which I will post to Woods Hole to-morrow. The N.Y.Times did reply with your pre-addressed card and stated that they will send the paper to Woods Hole August 1st through 31st. A copy of Punch arrived for Esther with no note as to sender, possibly Clive or Bruce but the writing of the address did not agree with either.

As yet there is no further news on the rebuilding front but I understand that Mr.Rowland will be coming tomorrow to spend a day or so replanning his drawings.

The V story has not progressed very far, I'm having difficulty getting a phage preparation from SL 18, or rather I have not been able to obtain one to which SL 15 is sensitive. On retesting the 'motilised' SL 15 cultures, after subculture on NSA rather than EMB, they all seem to be IV,V, even those which are non-lysogenic but motile. The numbers are very small and this must be extended. In any case there is selection for those cells in which it is known a change has occured. It would be more logical to pick colonies at random after adsorption of, say, FA 22 to SL 15 and then to test these, after purification, for V, motility, and lysogenicity. This I will do if I fail to obtain a suitable preparation from SL 18.

I hope your stay at Woods Hole proves profitable, at least it will be more bearable than Madison's humidity.

Sincerely,

Alec